

## Spillovers from Costly Credit

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### Abstract

Recent studies of credit access among low-income households find that payday loans can exacerbate, rather than alleviate, financial distress. I find that households with payday loan access are also more likely to use food assistance benefits and less likely to make child support payments required of non-resident parents. These findings suggest that borrowers in distress turn to transfer programs to supplement the household's resources and prioritize loan payments over other liabilities like child support payments. In that way, borrowing produces negative externalities, with both taxpayers and non-resident family members bearing a cost due to payday lending.

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## I. Introduction

The expansion of credit to low- and moderate-income households in the United States was a notable development of the 2000s, with substantial growth not only in mortgage credit but also in short-term, unsecured credit such as overdraft loans provided by banks and cash advances provided by payday lenders. Subprime mortgage originations roughly tripled in the 2000s, growing from 7.5% of annual mortgage originations in 2001 to over 20% by 2006 (Furlong and Krainer 2007). Similarly, payday loan volume grew six-fold in the first half of the decade, increasing from \$8 billion in 1999 (Stegman 2007) to an estimated \$48.5 billion in 2006 (Stephens 2011).

When assessing the economic efficiency of these credit expansions, it is important to consider whether the increase in household indebtedness entailed social costs that borrowers and lenders failed to internalize in making their choices.<sup>1</sup> Recent research on the housing market highlights substantial negative externalities related to homeowners' increased leverage and risk of default, costs borne by taxpayers who have funded mortgage modification subsidies (CBO 2013), and by neighbors whose home values have declined with nearby foreclosures.<sup>2</sup> The goal of this paper is to examine the importance of external costs in short-term credit markets by testing whether the rise of payday lending affected households' participation in transfer programs funded by taxpayers and households' payment of child support to non-resident family members.

There has been considerable debate about whether payday lending eases or encourages household financial distress. In principle, access to credit can improve welfare by allowing households to smooth expenditures through periods of income and consumption shocks.

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<sup>1</sup> Of course, externalities are just one consideration when assessing the efficiency of credit supply. As Zinman (2014) highlights, a complete evaluation requires examination of lenders' market power, borrowers' preferences and the impact of information differences between borrowers and lenders.

<sup>2</sup> A number of papers document price declines resulting from nearby foreclosures (Immergluck and Smith 2006; Harding et al. 2009; Campbell, Giglio and Pathak 2011; Mian, Sufi and Trebbi 2011; Gerardi et al. 2012).

However, payday loans are also quite expensive, with a typical bi-weekly fee of \$15 per \$100 borrowed. Given these high costs, facilitating loans to increase current consumption can create hardship in the future, especially among individuals with cognitive biases, forecasting problems (Ausubel 1991; Brunnermeier and Parker 2005; Bond, Musto and Yilmaz 2009) or self-control problems (Laibson 1997).

Though results vary across empirical studies, there is evidence that the expansion of payday credit aggravates financial difficulties, at least for a subset of borrowers. Rather than allowing households to pay important bills and forestall bankruptcy, payday loans increase their difficulty in paying mortgage, rent and utilities bills (Melzer 2011) and their likelihood of filing bankruptcy (Skiba and Tobacman 2011). Among military personnel, payday loan access worsens job performance (Carrell and Zinman 2014). Furthermore, financial literacy is particularly low among payday borrowers (Lusardi and Tufano 2009; Lusardi and Scheresberg 2013), and simple information disclosures designed to lessen cognitive biases and limitations result in significant reductions in payday borrowing (Bertrand and Morse 2011). Collectively, this evidence suggests that loan access can be costly rather than beneficial for some households and that behavioral biases may contribute to sub-optimal borrowing choices.<sup>3</sup>

This study probes more deeply into who bears these costs. Using household survey data from the Census' Survey of Income and Program Participation (SIPP) and following the identification strategy of Melzer (2011), I test for externalities using two outcomes that are measurable in survey data: food stamp participation and child support payments. The empirical

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<sup>3</sup> Studies of payday lending are not uniform in their conclusion that loan access aggravates financial hardship. The disparity in findings may reflect heterogeneity in the effects of loan access, both across borrowers and across states of the world; certainly usage varies substantially across borrowers. Caskey (2010) provides an overview of this literature. Beneficial effects of payday lending include: improvement to households' subjective financial well-being (Zinman 2009), reduction in foreclosures following natural disasters (Morse 2011), lower incidence of bounced checks (Morgan, Strain and Seblani 2012) and less consumption variability between paychecks (Zaki 2014), particularly for long pay cycles (Parsons and Van Wesep 2013). Two studies find no effect of payday lending on credit scores (Bhutta 2014; Bhutta, Skiba and Tobacman 2014).

strategy exploits differences in loan access that result from households' ability to borrow across state borders. Focusing on states that prohibit payday loans, I compare households that live close to a payday-allowing state, who can readily borrow by crossing the border, to households that live further from such borders and face more limited access.<sup>4</sup> By measuring loan access in this way, the research design avoids potential biases due to lenders' endogenous choice of location and allows for fixed effects to absorb state-level differences in food stamp participation and child support payments. The latter feature is crucial, since states differ substantially in their implementation of the food stamps program and their enforcement of child support orders.

Confirming results from Melzer (2011) within the larger SIPP sample, I find that households with proximate access to payday loans are more likely to report economic hardship, particularly in the years during which payday lending was widespread. The probability of economic hardship – as measured by the household's failure to pay important bills (mortgage, rent and utilities) or obtain needed medical care – increases by 4 percentage points, or 16%, where payday loans are available.

These increases in economic hardship are also manifest in households' use of food stamps: the probability of food stamp participation increases by 5 percentage points in areas with loan access, a 16% increase relative to the average usage rate in the sample. This difference does not reflect general economic disadvantage: households with proximate loan access, if anything, appear to have slightly higher incomes on average. Digging more deeply into the cross-sectional correlation between payday lending and program participation also reveals a time pattern consistent with a causal relationship. Specifically, the correlation between payday lending and

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<sup>4</sup> Industry data reveals substantial cross-border borrowing among residents of Georgia, where payday loans are prohibited (See "Georgia Border Residents ..." 2007). Payday lenders also locate in greater numbers at the borders of prohibiting states, consistent with lenders serving cross-border borrowers (Melzer 2011; Bhutta 2014).

food stamp receipt is absent in the early- to mid-1990s, before payday loans were available, and the correlation strengthens through the 2000s as payday lending gains further penetration.

Lastly, for child support payments I find greater incidence of payment delinquency among households with proximate access to payday loans, both as reported by child support payers and child support recipients. Although households with loan access are no more likely to have a child support obligation, they are 9.5 percentage points less likely to make a payment conditional on having an obligation, which represents a 12% decline relative to the average incidence in the sample. Reports by child support recipients confirm the same pattern: recipients living close to payday lending receive less support than they are owed, particularly when the payer lives nearby and therefore also has access to payday lending. In cases where the payer lives further away, there is no difference in payments received. In that way, the analysis rules out bias due to an omitted neighborhood variable, as the relationship between loan access and child support delinquency is not a fixed feature of recipients that live close to payday borders. Rather, the relationship varies among recipients depending on the payer's proximity to payday lending, as one would expect.

These findings indicate that as borrowers accommodate interest and principal payments on payday loan debt, they turn to transfer programs like food stamps to supplement the household's resources and they prioritize loan payments over other liabilities like child support payments. In that way, payday lending entails negative externalities that, unless offset by other market distortions, result in excess supply of credit.<sup>5</sup>

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<sup>5</sup> Dobbie and Skiba (2013) find ambiguous effects of information asymmetry on credit supply in the payday loan market. They find evidence of adverse selection – borrowers seeking larger loans default more often – which may lead to credit rationing. Yet they also find that increasing the size of a loan for a given borrower reduces his default risk (the opposite of moral hazard), which induces the opposite effect, pushing lenders to supply more credit.

By characterizing externalities from borrowing, this study extends the academic literature on payday lending, which has largely focused on the costs and benefits of loan access for borrowers themselves.<sup>6</sup> The study also complements research on child support payments by showing that household borrowing influences payment delinquency. Prior research finds that payment delinquency becomes more prevalent as child support debt increases (Heinrich, Burkhardt and Shager 2011; Cancian, Heinrich and Chung 2013), and the current study indicates that other types of indebtedness can have the same effect.

## II. Background on Payday Lending, Nutrition Assistance Programs and Child Support

### II.A. Payday Lending

Payday loans are small, short-term loans that evolved from check cashing transactions as check cashers began to advance funds against personal checks (Stegman 2007). In a typical transaction, the borrower receives \$350 of cash in exchange for a promise to repay \$400 in two weeks. The transaction fee – in this example, \$50 – is typically around 15% of the loan amount. Underwriting is minimal: borrowers must have a bank account and must show a recent pay stub as evidence of employment.

A recent study by the Consumer Financial Protection Bureau (CFPB 2013) uses administrative records for a sample of 15 million payday loans to characterize borrowers and their usage patterns. Borrowers have low to moderate incomes: the median borrower has annual income of \$22,500 and the vast majority of borrowers have income below \$50,000. Most borrowers also have weak credit histories and limited access to credit from cheaper sources such as credit card loans (Bhutta et al. 2014; Elliehausen and Lawrence 2001). The median loan

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<sup>6</sup> Morse (2011) and Cuffe (2013) study the effect of payday lending on crime. They find mixed results on crime-related externalities: Morse (2011) finds that payday lending reduces crime following natural disasters and Cuffe (2013) finds that payday lending leads to higher crime rates.

provides \$350 of credit for two weeks in exchange for a fee of \$52.50, or 15% of the loan amount (CFPB 2013). In practice, however, payday loans are used for much longer than two weeks, as borrowers commonly renew or roll over loans (Carter, Skiba and Sydnor 2013). Almost 50% of borrowers use 11 or more loans per year and 14% of borrowers take out 20 or more loans per year (CFPB 2013). For these heavy uses, annual interest payments are substantial: the median borrower pays \$460 in fees per year and the top quartile of borrowers pays more than \$780 in fees per year (CFPB 2013).

The payday lending industry emerged in the mid- to late-1990s and grew substantially, peaking at 25,000 store locations in 2006 before declining to 19,600 locations in 2010. Annual loan volume is estimated to have grown in parallel, from about \$8 billion in 1999 (Stegman 2007) to \$48.5 billion in 2006, before declining to \$40 billion in 2010 (Stephens 2011). The industry is regulated at the state level, through check cashing, small loan and usury laws. Restrictions on fees, loan amounts and loan rollovers are typical, and several states have taken a more extreme position, enacting or enforcing a prohibition on payday lending (McKernan, Ratcliffe and Kuehn 2013). Prohibiting states are crucial in this study, which makes use of cross-border access within states that disallow payday lending. Over the study period, eight states maintained prohibitions for some period of time: Connecticut, Georgia, Maryland, Massachusetts, New Jersey, New York, North Carolina and Vermont.<sup>7</sup>

## II.B. Nutrition Assistance

The Supplemental Nutrition Assistance Program (SNAP) is a federally funded transfer program that assists qualifying households in purchasing food. The program began in 1964 as the

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<sup>7</sup> Connecticut, Massachusetts, New Jersey, New York and Vermont prohibited payday loans for the entire sample period. Georgia, Maryland and North Carolina enacted prohibitions within the sample period, in 2004, 2002 and 2006, respectively.

Food Stamp Program (FSP) and was re-titled as the Supplemental Nutrition Assistance Program in 2008.<sup>8</sup> To qualify for food stamps, a household must pass income and asset tests: those with income below 130% of the Federal Poverty Level (FPL) qualify, as long as their assets (excluding primary residence and retirement savings) do not exceed \$2,000. Currently, the poverty level for a family of four is \$23,850 per year, which means that households with annual income below \$31,005 qualify for food stamps as long as they lack \$2,000 in assets. The set of food stamp-eligible households overlaps strongly with the set of payday borrowers: more than two thirds of payday borrowers are below the income cutoff for food stamp eligibility (CFPB 2013).

Food stamps are an important component of the social safety net for low-income households.<sup>9</sup> Participation in the program is widespread – an average of 23 million people per year, or roughly 10% of the U.S. adult population, participated during this study’s sample period (1991 to 2007). And, for participating households, food stamps make up a significant share of their income. Using the current benefit schedule as a representative example, a family of four with monthly income of 100% FPL (\$1,988) would be entitled to \$204 in monthly benefits, which is more than 10% of its earned income.<sup>10</sup> Across all participating households year-to-date, the average monthly benefit is \$260.<sup>11</sup>

Participation in the food stamps program varies considerably over time and across states, due to both economic factors and policy differences (Ganong and Liebman 2013). For example, national caseloads fell dramatically in the late 1990s, from 25.5 million people in 1996 to 17.2

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<sup>8</sup> I refer to SNAP and FSP benefits interchangeably as “food stamps.”

<sup>9</sup> Blundell and Pistaferri (2003) and Gunderson and Ziliak (2003) find that food stamps facilitate consumption smoothing for low-income households, especially those that experience a persistent income shock.

<sup>10</sup> SNAP benefit payments increase with household size and decrease with income.

<sup>11</sup> Data on SNAP benefits and participation are reported at the U.S. Department of Agriculture website: <http://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap>

million people in 2000, due to a strengthening economy and changes to welfare policies (Currie and Grogger 2001; Ziliak, Gunderson and Figlio 2003). Across states, estimated participation rates also vary markedly at least in part because of state-level differences in program administration (Ratcliffe, McKernan and Finegold 2008). In 2011, the proportion of eligible individuals collecting benefits ranged from a low of 57% in California to a high of nearly 100% in Oregon, Washington and Maine (Cunnyngham 2014). An important feature of this study's empirical strategy, which exploits within-state variation in loan access while controlling for state-by-time fixed effects, is that it avoids bias due to unobserved heterogeneity in program participation over time and across states.

## II.C. Child Support

Child support orders are legal directives, established through a court or administrative hearing process, that mandate the financial support of children by nonresident parents. The rationale behind requiring child support is to ensure the well-being of children in single parent households. Especially for low-income recipients, support payments are an important source of income: for the average recipient, support payments constitute 16% of income (Grall 2013), and for recipients with income below the poverty level, support payments constitute one third of income (Sorensen and Oliver, 2002).

Despite being legal obligations that are not dischargeable in bankruptcy, child support obligations routinely go unpaid. Of the 6.3 million custodial parents due child support in 2011, only 43% received full payment, while 26% received no payment and 31% received partial payment (Grall 2013). In dollar terms, the \$23.6 billion of support that was paid in 2011 constituted less than two thirds of the \$37.9 billion that was due. This shortfall contributed to

accumulated child support debt of \$111 billion by the end of 2011 (Office of Child Support Enforcement 2014).

Policies governing award determination and enforcement differ substantially across states (Heim 2003), which results in stark differences in payment delinquency. In 2011, for example, only 51% of obligations were paid in Alabama, whereas 84% of obligations were paid in Pennsylvania (Office of Child Support Enforcement 2014). As discussed above, the empirical design will isolate within-state variation in loan access to avoid bias due to omitted state-level factors that might influence both child support enforcement and payday lending laws.

### III. Data and Research Design

#### III.A. Data

The Census Bureau's Survey of Income and Program Participation (SIPP) is the primary data source for this analysis. The SIPP is a longitudinal survey that tracks household well-being and public program participation over a period ranging from 32 to 48 months. During that time, participants report their monthly income, employment and transfer program participation through "core wave" interviews that recur every four months. Participants report additional information on economic hardship, wealth and child support payments, among other topics, at lower frequency. The initial sample ranges from 14,000 to 43,000 households (with smaller samples in earlier panels) and is designed to oversample low-income households for whom program participation is most likely, but is otherwise nationally representative and covers all 50 states.

This analysis uses the 1991, 1992, 1993, 1996, 2001 and 2004 SIPP Panels, which provide data for 1991 through the end of 2007. The analyses of economic hardship and child support use the Adult Well-being, Child Support Paid and Child Support Agreements Topical Modules, while the analysis of food stamps uses information from the higher frequency "core

wave” interviews. Detailed geographic information, which is used to measure distance to payday lending, is unavailable in the SIPP public use files, so the analysis uses non-public use data warehoused in the Census Research Data Center.

The SIPP data are merged with the following county-level data: economic and demographic information from the 2000 Census, monthly unemployment rates from the Bureau of Labor statistics and annual personal income information from the Bureau of Economic Analysis.

### III.B. Research Design

Identifying the causal impact of payday lending on household financial distress is challenging. Given that households seek payday loans particularly when they have fallen behind on other debt payments (Bhutta et al. 2014), it is difficult to disentangle whether borrowing is the cause of financial distress or the result. Furthermore, even after ignoring variation in loan *take-up* and focusing instead on loan *access*, the problem remains that lenders may choose where to locate in response to borrower demand and creditworthiness, resulting in correlation between borrower distress and loan access even in the absence of a causal effect.

To address this problem, the analysis focuses on residents of states that prohibit payday lending and gauges each household’s access to payday loans based on its distance from the nearest payday-allowing state. For the main analysis, the key independent variable is *Payday Access*, an indicator variable that takes a value of one for households located in counties less than 25 miles from a state that allows payday lending at that time, and zero otherwise. Importantly, *Payday Access* varies based on household location and the payday lending laws of bordering states, but is not affected by the endogenous location decisions of lenders. For use in falsification exercises that test for the absence of a loan access effect in the early years of the

sample, I also define *Payday Border*, an indicator for whether a household is within 25 miles of a state that allowed payday lending at any point during the sample period. For use in robustness exercises, I also create a continuous measure of payday loan access, *Log Distance Payday*, which is the natural logarithm of the distance to the nearest payday allowing state.

### III.C. Economic and Demographic Characteristics of Areas with Payday Loan Access

Table I displays sample statistics for a variety of economic and demographic variables, stratified by *Payday Access*.

County-level data, summarized in Panel A, indicates that *Payday Access* areas are more prosperous, with lower rates of unemployment and slightly higher per capita income: 4.5% unemployment rate and \$35,500 per capita income compared to 4.9% and \$34,400 among non-access counties. *Payday Access* areas are also less populous and more rural: the average 2000 Census population in an access area is 152,000 with 50% urban population, compared to an average population of 251,000 in non-access areas, 61% of which is in an urban area.

Summary statistics for low- to moderate-income SIPP respondents (between \$15,000 and \$50,000 in annual income) are shown in Panel B. Households with *Payday Access* have slightly higher monthly income (\$2,609 vs. \$2,570), higher rates of home ownership (63% vs. 53%), and lower rates of being uninsured (19% vs. 21%), but they show no difference in employment status. Educational attainment is modestly higher in *Payday Access* areas, as a larger proportion of households obtain at least a high school diploma (92% vs. 89%). Disparities in racial/ethnic composition are more striking, with fewer minorities residing in *Payday Access* areas: the proportion of black households is 13%, compared to 16% in areas without access, and the proportion of Hispanic households is 3%, compared to 11% in areas without access. Heads of household, on average, are older in areas with loan access (53.3 years vs. 51.6 years). In other

household characteristics, including number of children and household size there are only modest differences between access and non-access areas.

To summarize, areas with payday loan access appear to be more rural, less racially diverse and slightly more prosperous, which highlights the need for county-level and individual-level controls in the regression analysis that follows. Beyond controlling for these variables in the main analysis, I will also estimate the effect of loan access within subsets of the main sample (for each racial category and for the subset of heavily urban areas) to ensure a closer match between the treatment and comparison groups. Finally, I will use falsification exercises – testing for the absence of a *Payday Access* effect before loans are available – to evaluate whether county-level omitted variables lead to bias in the estimated effects of loan access.

#### IV. Regression Model and Results

The regression analysis uses the following model:

$$y_{icst} = \alpha + \beta \text{Payday Access}_{ct} + \delta \mathbf{X}_{it} + \theta \mathbf{Z}_{ct} + \gamma_{st} + \varepsilon_{icst},$$

where the subscripts indicate the household  $i$  in county  $c$  and state  $s$  at time  $t$ .  $\beta$  is the coefficient of interest. All specifications include state-year fixed effects, denoted by  $\gamma$ , so that  $\beta$  is not identified from cross-state differences in hardship or state-level changes in hardship over time. All models include a vector of household-level controls, denoted by  $X$ , and a vector of county-level controls, denoted by  $Z$ . The household-level controls are: natural logarithm of monthly income; number of HH members; number of children; age of the head of household; and indicator variables for unemployment, lack of health insurance, home ownership, educational attainment (5 categories) and race (4 categories). The county-level controls include three static measures from the 2000 Census – cubics in county median income, population and percent urban population – as well as *Border*, an indicator for whether the household is located within 25 miles

of any state border. The vector of county controls also includes two time-varying controls, the county's unemployment rate and the natural logarithm of per capita personal income. For the analysis of economic hardship and child support payments, the regression sample is limited to households with \$15,000 to \$50,000 of annual income, the range that encompasses the vast majority of borrowers. For the analysis of food stamp usage, the regression sample is limited to food stamp-eligible households, i.e. households with income below 130% of the federal poverty level and non-housing assets below \$2,000. Standard errors are estimated with observations clustered by county.

#### IV.A. Payday Loan Access and Economic Hardship

The first phase of the analysis examines the relationship between payday loan access and economic hardship, replicating results from Melzer (2011) using the SIPP sample. The SIPP's survey questions are quite similar to those of the National Survey of America's Families (NSAF), which is studied in Melzer (2011). However, the SIPP includes a longer time series and greater geographic coverage, which increases the number of prohibiting states analyzed from three in that study to eight in this study.<sup>12</sup>

To measure economic hardship, the SIPP includes a battery of questions about the household's ability to afford essential items and pay important bills over the prior four or twelve months. The questions about food-related hardship, which are of particular interest, are coded into two indicator variables. *Cut Meals (adult)* takes the value of one if over the prior four months the adults cut the size of meals or skipped meals due to lack of money. Analogously, *Cut Meals (children)* is one if the children were not eating enough because the household couldn't afford more food, or zero otherwise. The questions about hardship on non-food expenditures are

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<sup>12</sup> The SIPP sample covers 1991 to 2007 whereas the NSAF sample covered 1996 to 2002. The SIPP survey also samples households in all states, whereas the NSAF sampled primarily households in thirteen "focal" states.

summarized in a single indicator variable, *Any Hardship*, which takes the value of one if the household was unable to pay its mortgage, rent or utilities bills or if anyone in the household failed to get needed dental or medical care over the prior twelve months.

The regression results, shown in Table II, reveal higher incidence of economic hardship in *Payday Access* areas. Households with cross-border loan access are 4.0 percentage points more likely to report hardship on the summary measure. This difference is statistically significant at the 5% level and represents a 16% increase relative to the average incidence of 24.9% across all households in the sample. In contrast, the measures of food-related hardship display smaller and statistically insignificant coefficients on *Payday Access*. The point estimate for *Cut Meals (Adult)* indicates higher incidence in *Payday Access* areas (a 41 basis point, or 9%, increase), but without statistical significance, and *Cut Meals (Children)* shows little relationship with *Payday Access*. The difference in economic hardship is particular to borders between payday-prohibiting and payday-allowing states; the regression results suggest little difference in border areas in general, with the coefficient on *Border* being slightly negative and insignificant. These findings regarding *Payday Access* – greater difficulty in paying important bills, but smaller and statistically insignificant differences in food-related hardship – match the results of Melzer (2011) in the NSAF sample.

The effect of payday lending on economic hardship also displays the time pattern that one would expect. As shown in Table III, geographic proximity to payday lending has a small and statistically insignificant effect on hardship in 1992 and 1995, before payday loans were available. These additional results are informative, as they confirm that the main findings do not reflect a persistent difference in hardship in these particular border areas that pre-dates payday lending. It is only in the mid-2000s, when payday loan stores were prevalent, that *Payday Access*

predicts greater rates of economic hardship. Specifically, those with proximate access to payday lending are 7.6 percentage points and 4.9 percentage points more likely to report economic hardship in 2003 and 2005, respectively.

#### IV.B. Payday Loan Access and Food Stamp Participation

The second phase of the analysis examines the relationship between payday loan access and food stamp utilization, using both an indicator variable for food stamp receipt (*FS Received*) and a continuous variable (*FS Amount*) for the dollar amount of benefits received. To ensure accuracy in these measures by avoiding “seam bias,” I follow common practice among others using the SIPP by dropping observations outside of the interview month (Moore 2008).<sup>13</sup> As a result, the dependent variable is a measure of monthly food stamp usage at a frequency of every four months.

The regression results, which are shown in Table IV, indicate that food stamp usage is higher among food-stamp eligible households with payday loan access. Among eligible households, food stamp receipt is 5 percentage points more prevalent in payday access areas, a 16 percent increase relative to the average take-up of 31%.<sup>14</sup> The value of benefits received shows a similar difference: monthly benefits are higher by \$10.10 for households with proximate loan access, a 17% increase over the \$61.10 monthly average in the regression sample. As with economic hardship, this finding is conditional on individual-level controls and county-level economic controls, as well as the border indicator. Again, the *Payday Access* effect does not reflect a general difference in border areas; the coefficient on the border indicator is small and

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<sup>13</sup> Transitions in employment and program participation are reported at much greater frequency at seams between interviews than during interview reference periods, perhaps because households recall the recent month most clearly and repeat their response for prior months for which their memory is less clear.

<sup>14</sup> The food stamp participation rate within this sample is similar to the participation rate calculated by others using household survey data (Gunderson and Ziliak 2003). Consistent with Meyer, Mok and Sullivan (2009), who document systematic underreporting of transfer program participation in household surveys, I find lower rates of participation and lower average benefit levels than administrative data imply.

insignificant in both models. These findings for food stamp receipt may help to explain why measures of food-related hardship – cutting back on quantity or quality of meals due to lack of money – show no significant increase with payday lending despite increased hardship along other dimensions; borrowers that would otherwise cut back on food consumption may avoid doing so by taking up food stamps.<sup>15</sup>

The analysis presented in Table V explores the robustness of these results. The first model, which is estimated with sample weights, confirms that adjusting for differences in sampling probability has little effect on the estimated *Payday Access* coefficient. The second model uses an alternative measure of payday loan access, *Log Distance Payday*. This model confirms the original finding, as food stamp receipt declines with (log) distance to the nearest payday-allowing state. The final four models estimate *Payday Access* coefficients using subsets of the original sample, with the particular sub-samples chosen to limit variation in characteristics that differ, on average, between *Payday Access* and non-*Payday Access* areas. Within racial sub-samples, food stamp receipt still increases with *Payday Access*, with somewhat smaller proportional increase for whites (2.7 percentage points relative to 24% average take-up) than for blacks (7.5 percentage points relative to 46% average take-up). Focusing on counties with greater than 90% urban population, the *Payday Access* coefficient remains positive and significant. Finally, the payday access effect remains positive and significant, even after dropping observations from smaller counties (those with population less than 500,000).

Table VI shows how the relationship between *Payday Access* and food stamp receipt varies over time. In these regressions, the main sample is divided into five three-year intervals between 1991 and 2005 and one two-year interval (2006-2007) at the end of the sample. The

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<sup>15</sup> Hoynes and Schanzenbach (2009) find that food consumption increases with food stamp take-up. Ratcliffe, McKernan and Zhang (2011) and Mabli, Ohls, Dragoset, Castner and Santos (2013) find that food stamp take-up reduces food-related hardship.

results show that payday loan access has little effect on food stamp participation in the early years of the sample, but a strong and statistically significant effect in the later years, once payday loans are widely available. For the sample period in the early- to mid-1990s, the *Payday Border* coefficients are small and statistically insignificant, confirming that households in future payday-access areas do not differ in food stamp participation before loans were available. Between 1997 and 1999, after the entry of payday lenders but before stores were prevalent, there is no significant relationship between *Payday Access* and food stamp utilization. The point estimate is negative, but consistent with a wide range of effects. Finally, throughout the 2000s the estimated effects of *Payday Access* are positive and strongly significant, consistent with a robust effect of payday lending during the time when payday loan stores were most prevalent.

#### IV.C. Payday Loan Access and Child Support Payments

The final stage of the analysis studies child support payments, both from the perspective of payers and recipients. Table VII presents the regression analysis for child support payments as reported by payers. Roughly 4% of sample households report that they are required to make child support payments, with no significant difference based on *Payday Access*. Though the requirement to pay child support does not vary with payday loan access, delinquency does. As shown in the second column, households with proximate access to payday loans are 9.5 percentage points less likely to make a payment, a 12 percent decline relative to the sample average of 79% that report making their required payments. As shown in the third and fourth columns, estimating the model using least squares with sample weights or probit has little effect on the *Payday Access* coefficient estimate, though the standard errors do change. The main results are also robust to using *Log Distance Payday* as the key independent variable; the point estimate on *Log Distance Payday* is positive, implying that the probability of payment is lowest

among those close to a payday-allowing state and increases with households' distance from payday lending. Prior to the 1996 Panel of the SIPP, respondents were not asked whether they were required to pay child support, so the data do not allow for a test of whether child support delinquency varied with proximity to a payday border before 1996.

One might be concerned that respondents do not accurately report their child support obligations and payments, so it is also worthwhile to examine child support delinquency from the perspective of recipients. Within the SIPP, child support recipients report the amount of child support that they receive relative to what they are owed as well as partial information on the location of the payer (whether the payer lives in the recipient's state and county). Of course, it is the payer's proximity to payday lending that is relevant in judging the effects of payday loan access on support payments.

Table VIII presents further results for child support payments, this time from the perspective of the recipient. The dependent variables in Panels A and B, respectively, are whether the recipient receives less child support than they are owed and the proportion of child support received relative to the amount owed. The first column in both panels shows the *Payday Access* coefficients estimated for all recipients. For both dependent variables, the point estimates suggest that delinquency is higher in areas with payday loans access, but the estimates are not significant. For this analysis, however, there is measurement error in loan access that attenuates the estimated effect: there are respondents in *Payday Access* counties for whom the payer lives elsewhere, in a county without proximate access, as well as respondents in non-*Payday Access* counties for whom the payer lives in a county with proximate access. To address this measurement problem, the second model refines the analysis by focusing on the sub-sample for which the child support payer's location is known more precisely. For this sub-sample –

encompassing cases in which the payer lives in the same county as the recipient – the *Payday Access* coefficients are larger and more statistically significant. For both dependent variables, delinquency rises with *Payday Access*: the likelihood of receiving less support than owed increases by 12 percentage points, or 20% relative to the sample mean, and the proportion of support received also declines by 12 percentage points, or 25% relative to the sample mean.

The final regression model exploits variation in child support payers' locations, holding fixed recipients' locations, to better identify the causal effect of loan access. For this analysis, the regression sample is limited to cases in which the recipient and payer live in the same state. Along with *Payday Access* and the full set of control variables, the regression model also includes an interaction between *Payday Access* and an indicator for whether the payer lives in the same county. This model provides separate estimates for the effect of living near a payday border depending on the location of the payer. The results, presented in the last column of Table VIII, show very small and statistically insignificant effects of *Payday Access*, which means that recipients living in payday access areas generally receive similar amounts of child support as recipients in other areas. It is only when the child support payer also has payday loan access (i.e., lives in the same county) that their support payments fall short of what they are owed, as indicated by the larger, statistically significant coefficients on the interaction term. Thus, elevated rates of child support delinquency are not a common feature of families in areas proximate to payday lending, as one might expect if a county-level omitted variable were confounded with payday lending. Rather, delinquency appears within the county precisely among those for which the child support payer has access to loans.

## V. Conclusion

Recent studies of credit access among low-income households find that high-cost payday loans can exacerbate financial distress for some borrowers, leading to increased difficulty paying mortgage, rent and utilities bills (Melzer 2011), increased risk of bankruptcy (Skiba and Tobacman 2011) and performance declines on the job (Carrell and Zinman 2014). This paper asks whether payday lending affects others outside the borrowing household. To gauge such costs or benefits, I examine child support payments and food stamp participation, and find that households with proximate access to payday lending are less likely to pay required child support and more likely to use food stamps, particularly during the time period when payday lending is prevalent.

This evidence suggests that households struggling to meet debt payments on payday loans adapt by reducing child support payments and by seeking transfer benefits to supplement household income. In both ways, the impact of payday lending spreads beyond the borrowing household and results in negative externalities. These social costs should be weighed alongside the direct costs and benefits of credit access for borrowers when evaluating policies that impact the supply of short-term credit.

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**Table I: Summary Statistics**

	Payday Access = 0		Payday Access = 1		Difference Significant at 5%
	obs	mean	obs	mean	
<i>Panel A: County characteristics</i>					
Unemployment rate (%)	8,368	4.89	3,317	4.45	*
Personal income per capita	8,368	34,387	3,317	35,542	*
Median income	163	43,020	113	41,719	
Population	163	251,344	113	151,857	*
Percent urban population	163	61	113	50	*
<i>Panel B: Household characteristics</i>					
Income/Assets					
Monthly income	44,000	2,570	14,000	2,609	*
Homeowner?	44,000	0.53	14,000	0.63	*
Employment/Insurance					
Unemployment spell?	44,000	0.05	14,000	0.05	
Uninsurance spell?	44,000	0.21	14,000	0.19	*
Education					
Less than HS	44,000	0.12	14,000	0.09	*
HS diploma	44,000	0.34	14,000	0.34	
Some college	44,000	0.32	14,000	0.36	*
College	44,000	0.15	14,000	0.14	*
Graduate	44,000	0.08	14,000	0.08	
Race/Ethnicity					
White	44,000	0.70	14,000	0.80	*
Black	44,000	0.16	14,000	0.13	*
Hispanic	44,000	0.11	14,000	0.03	*
Asian/Other	44,000	0.04	14,000	0.04	*
Other					
Age	44,000	51.6	14,000	53.3	*
HH size	44,000	2.37	14,000	2.24	*
Number of children	44,000	0.60	14,000	0.55	*

Notes: Reported above are county-level and household-level summary statistics, stratified by Payday Access, an indicator for proximity to payday lending. The sample in Panel A includes counties within states that prohibit payday lending. The sample in Panel B includes households with annual income between \$15,000 and \$50,000 that live in states that prohibit payday lending. Due to Census disclosure constraints, the number of observations in the household sample is rounded down to nearest thousand.

**Table II: Economic Hardship and Payday Loan Access**

Dependent Variable:	Any Hardship	Cut Meals (Adults)	Cut Meals (Children)
Mean:	0.249	0.046	0.003
Payday Access	0.040** (0.020)	0.004 (0.009)	-0.0001 (0.001)
Border	-0.008 (0.007)	-0.005 (0.003)	-0.001 (0.001)
Log(Household Income)	-0.13*** (0.01)	-0.049*** (0.004)	0.003*** (0.001)
Unemployment spell	0.12*** (0.01)	0.043*** (0.01)	0.01*** (0.003)
Uninsured spell	0.16*** (0.01)	0.026*** (0.003)	0.001 (0.001)
N	37,000	40,000	40,000
R <sup>2</sup>	0.15	0.05	0.01

Notes: Reported above are OLS estimation results for regressions of economic hardship on an indicator for proximity to payday lending (*Payday Access*). In addition to the covariates listed in the table, all specifications include state-year fixed effects, household-level controls and county-level controls. The additional household-level controls are: number of HH members; number of children; age of the head of household; and indicator variables for home ownership, educational attainment (5 categories) and race (4 categories). The additional county-level controls are: cubics in median income, population and percent urban population from the 2000 Census; county unemployment rate from the Bureau of Labor Statistics; and the natural logarithm of county per capita personal income from the Bureau of Economic Analysis. The regression sample includes all households with \$15,000 to \$50,000 of annual income for the years 1996 and later. Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table III: Economic Hardship and Payday Loan Access Over Time**

Dependent Variable:	----- Any Hardship -----				
Sample Year:	1992	1995	1998	2003	2005
Mean:	0.23	0.17	0.24	0.24	0.27
Payday Access			-0.011 (0.043)	0.076** (0.031)	0.049* (0.028)
Payday Border	0.011 (0.023)	-0.011 (0.034)			
N	14,000	7,000	12,000	9,000	14,000
R <sup>2</sup>	0.16	0.12	0.15	0.15	0.15

Notes: This analysis explores the relationship between economic hardship and loan access through time, with a separate regression for each year in which hardship is measured in the SIPP. Reported above are OLS estimation results for regressions of economic hardship on an indicator for proximity to payday lending (Payday Access or Payday Border). Payday Access is coded based on the household's proximity to a state that allows payday lending at the time of the interview. Payday Border is coded based on the household's proximity to a state that allows payday lending not just at the interview, but at any time between 1991 and 2007. All specifications include state-year fixed effects and the full set of household- and county-level controls reported in Table II. The regression sample is restricted to households with \$15,000 to \$50,000 of annual income. Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table IV: Food Stamp Usage and Payday Loan Access**

Dependent Variable:	FS Received	FS Amount
Mean:	0.31	61.1
Payday Access	0.050** (0.018)	10.1* (5.5)
Border	0.006 (0.007)	0.1 (1.6)
Ln(Income)	-0.001 (0.002)	-6.5*** (0.5)
Unemployment spell	0.076*** (0.006)	31.4*** (1.7)
Uninsured spell	-0.110*** (0.006)	-21.1*** (1.6)
N	183,000	183,000
R <sup>2</sup>	0.22	0.30

Notes: Reported above are OLS estimation results for regressions of food stamp usage on an indicator for proximity to payday lending (Payday Access). Food stamp usage is measured first as an indicator of receipt within the month (FS Received) and then as the dollar amount of benefits received in the month (FS Amount). In addition to the covariates listed in the table, all specifications include state-year fixed effects, and the same set of household- and county-level controls included in the analysis of economic hardship (described in Table II). The regression sample includes all food stamp-eligible households (monthly income below 130% of the Federal Poverty Level and assets of \$2,000 or less). Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table V: Food Stamp Usage and Payday Loan Access, Robustness**

Dependent Variable:	----- FS Received -----					
Sample:	All Households	All Households	Whites	African-Americans	Counties with at least 90% Urban Pop.	Counties with at least 500K Pop.
Estimation Method:	OLS (weighted)	OLS	OLS	OLS	OLS	OLS
Mean:	0.29	0.31	0.24	0.46	0.31	0.31
Payday Access	0.043** (0.018)		0.027 (0.018)	0.075** (0.031)	0.061** (0.026)	0.120** (0.050)
Log Distance Payday		-0.024** (0.010)				
N	183,000	183,000	107,000	40,000	83,000	71,000
R <sup>2</sup>	0.23	0.22	0.22	0.26	0.26	0.26

Notes: This analysis explores the robustness of the relationship between food stamp usage and payday lending. Reported above are estimation results for regressions of an indicator of food stamp receipt within the month (FS Received) on an indicator for proximity to payday lending (Payday Access). The first model is estimated using sample weights. The second model replaces Payday Access with a continuous measure of proximity to lenders, the log of the household's distance to a payday-allowing state (Log Distance Payday). The final four models are estimated in subsets of the main sample, restricting the sample first based on race, then based on the proportion of urban population in the county and finally based on the county population. In addition to the covariates listed in the table, all specifications include state-year fixed effects, and the same set of household- and county-level controls included in the main analyses of economic hardship (Table II) and food stamp usage (Table IV). In each specification, the regression sample is restricted to food stamp-eligible households (monthly income below 130% of the Federal Poverty Level and assets of \$2,000 or less). Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table VI: Food Stamp Usage and Payday Loan Access Over Time**

Dependent Variable:	----- FS Received -----					
Sample Years:	1991-1993	1994-1996	1997-1999	2000-2002	2003-2005	2006-2007
Mean:	0.35	0.35	0.30	0.27	0.33	0.35
Payday Access			-0.050 (0.046)	0.063** (0.029)	0.100*** (0.022)	0.062** (0.030)
Payday Border	0.024 (0.021)	0.006 (0.018)				
N	49,000	48,000	50,000	33,000	56,000	24,000
R <sup>2</sup>	0.28	0.27	0.22	0.22	0.22	0.22

Notes: This analysis explores the relationship between food stamp usage and loan access through time, with a separate regression for each 3-year interval in the sample (the final sample covers a 2-year interval, 2006-2007). Reported above are OLS estimation results for regressions of an indicator for food stamp receipt within the month (FS Received) on an indicator for proximity to payday lending (Payday Access or Payday Border). Payday Access is an indicator variable measuring proximity to a state that allows payday lending at the time of the interview. Payday Border is an indicator variable measuring proximity to a state that allows payday lending not just at the interview, but at any time between 1991 and 2007. All specifications include state-year fixed effects and the same set of household- and county-level controls included in the main analyses of economic hardship (Table II) and food stamp usage (Table IV). In each specification, the regression sample is restricted to food stamp-eligible households (monthly income below 130% of the Federal Poverty Level and assets of \$2,000 or less). Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table VII: Child Support Paid and Payday Loan Access**

Dependent Variable:	CS Obligation	CS Paid	CS Paid	CS Paid	CS Paid
Estimation Method:	OLS	OLS	OLS (weighted)	Probit	OLS
Mean:	0.04	0.79	0.80	0.79	0.79
Payday Access	-0.002 (0.003)	-0.095 (0.058)	-0.092 (0.07)	-0.096* (0.042)	
Log Distance Payday					0.057* (0.032)
N	98,000	4,000	4,000	4,000	4,000
R <sup>2</sup>	0.54	0.18	0.17	0.14	0.18

Notes: This analysis explores the relationship between payday loan access and child support payments, as reported by payers. The first model regresses an indicator of whether the household has a child support obligation (CS Obligation) on an indicator for proximity to payday lending (Payday Access), controlling for marital status. The estimation sample includes all households with \$15,000 to \$50,000 of annual income. The final four models regress an indicator for whether the household paid child support (CS Paid) on measures of proximity to payday lending (Payday Access or Log Distance Payday) within the sample to households that have a child support obligation (and \$15,000 to \$50,000 of annual income). All specifications include state-year fixed effects, and the same set of household- and county-level controls included in the analyses of economic hardship (Table II) and food stamp usage (Table IV). The regression sample spans 1996 through 2007. Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest thousand.

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level

**Table VIII: Child Support Received and Payday Loan Access**

		Panel A		
Dependent Variable:		CS Received < Owed		
Sample:	All Households Owed	Payer in Same County	Payer in Same state	
Mean:	0.64	0.60	0.62	
Payday Access	0.06 (0.05)	0.12* (0.07)	0.01 (0.06)	
Payday AccessX Payer Same County			0.11** (0.05)	
N	6,000	2,000	4,000	
R <sup>2</sup>	0.07	0.12	0.09	

  

		Panel B		
Dependent Variable:		CS Proportion Received		
Sample:	All Households Owed	Payer in Same County	Payer in Same state	
Mean:	0.43	0.47	0.45	
Payday Access	-0.04 (0.04)	-0.12** (0.06)	-0.01 (0.05)	
Payday AccessX Payer Same County			-0.09* (0.05)	
N	6,000	2,000	4,000	
R <sup>2</sup>	0.08	0.13	0.10	

Notes: This analysis explores the relationship between payday loan access and child support payments, as reported by recipients. The dependent variable in Panel A is an indicator for whether the household received less child support than they were owed (CS Received < Owed). The dependent variable in Panel B is the amount of child support received as a proportion of the amount owed. Proximity to payday lending (Payday Access) is measured based on the location of the child support recipient. The estimation sample in the first specification includes all households owed child support, regardless of the child support payer's location. The second and third specifications limit the sample to recipients for which the payer is located in the same county or the same state, respectively. In order to test whether child support receipt depends on the payer's proximity to payday lending, the third model includes an interaction between the measure of the recipient's proximity to payday lending (Payday Access) and an indicator for whether the payer lives in the same county (Payer Same County). This model includes both Payer Same County and Payday Access as covariates. All specifications include state-year fixed effects, and the same set of household- and county-level controls included in the analyses of economic hardship (Table II), food stamp usage (Table IV) and child support paid (Table VII). In all specifications, the estimation sample spans 1996 through 2007 and is restricted to households with \$15,000 to \$50,000 of annual income. Standard errors, estimated with observations clustered by county, are reported in parentheses. Due to Census disclosure constraints, the number of observations in each regression is rounded down to nearest

\* Significant at the 10% level; \*\* Significant at the 5% level; \*\*\* Significant at the 1% level